volume of the "Principles," Lyell not only rejected the theory of Lamarck, but went far towards abandoning, for the time, any idea of "the transmutation of species."

It is scarcely necessary here to recall the fact that this second volume of the "Principles," so full of discussions bearing on the changes in organic life, reached Darwin in South America, just at the time when he was startled by discovering the relations between the living and recently extinct mammals of that continent. From that time forth Darwin no longer regarded the question of evolution with indifference. In the critical period between the return of the *Beagle*, in 1836, and the writing of the first sketch of the theory, in 1842, constant intercourse took place between the two friends: "I saw more of Lyell," says Darwin in his autobiography, "than of any other man, both before and after my marriage" (in 1839). In their frequent discussions, Darwin would become fully acquainted with the arguments of Cuvier and his school, which are, indeed, very clearly and trenchantly reproduced in the first three chapters of the second volume of the "Principles," which Darwin called his "own true love."

These facts borne in mind, I think we can have no difficulty in realising the source of the statements made by Darwin. I think the sentences may be paraphrased as follows:—

"Anti-evolutionists *admit* the inheritance of *small* variations. Well, the inheritance of such small variations is all I require for my theory of Natural Selection. I can afford to concede the non-inheritance of the greater variations."

But it is interesting to notice that in the sentence about plants and sea-shells following the passage in question, and in his discussion of the appearance and inheritance of a *sixth digit* in man, &c., Darwin was not satisfied that *only* small variations were transmitted.

It was the remembrance of facts like these that led me to suggest that the subject was "constantly present" in Darwin's mind. Prof. Meldola, thinking of the more acute discussion of the question aroused in 1885 by Weismann's declaration that *no* acquired characters are inherited, naturally expressed doubt on the subject, and I, of course, admit that this phase of the question, in all probability, never presented itself to Darwin, or at least never demanded his serious consideration.

Kew. JOHN W. JUDD.

The Transference of Names in Zoology.

As the preparation of an official list of *Nomina conservanda* is now under consideration by the International Commission on Zoological Nomenclature, it may not be out of place to direct attention to a point that seems to me of prime importance in this connection, although it has received little notice from recent writers on nomenclatural reform.

It is simply this—while the rejection and replacement of familiar names for well-known animals is, of course, an inconvenience to zoologists, it is a trivial matter in comparison with the grave possibility of confusion that arises when the names are used in an altered sense. In the former case we merely multiply synonyms, and, unfortunately, they are so numerous already that a few more hardly matter; in the latter case there is a real and serious danger of ambiguity. Thus, at present, a writer who mentions Trichechus may be referring either to the walrus or the manatee, Simia may mean either the orang or the chimpanzee, Cynocephalus may be either a "flying lemur" or a baboon, and so on through all the great groups of the animal kingdom until we come to Holothuria, which may refer either to a sea-cucumber or to a Portuguese man-of-war. Cases like these seem to me to be on an entirely different plane as regards practical importance, from those in which an old name is simply rejected; even if the shore-crab is to be called Carcinides for the future, we have only the additional burden of remembering that it was once called Carcinus.

A striking (if somewhat exceptional) instance of the pitfalls that are in preparation for future students is found in the section on Crustacea in Bronn's "Thierreich" (Bd. v., Abth. ii.). On p. 1056 there is an allusion to "Astacus," and on the following page to "Astacus (=Homarus)." In the bound volume (unless the part-wrappers have been kept in place) there is nothing to show that a change of authorship intervened between those two pages, and that, while the second "Astacus" refers to the lobster, the first indicates the crayfish.

If the International Commission could be persuaded to consider first those names that are threatened with *transference*, before proceeding to deal with those that are merely in danger of *replacement*, they would, I believe, secure the support and cooperation of many zoologists who have doubts as to the practicability of the schemes lately put forward. W. T. CALMAN.

British Museum (Nat. Hist.), Cromwell Road, London, S.W., January 23.

Sex Relationship.

IT seems a pity that writers should allow their political bias to influence their work, and especially that they should not at least ascertain the facts of a case before writing about it.

In his article on "Sex Relationship" in NATURE of January 5, Dr. R. J. Ewart said, in commenting on the present excess of females over males:—"The result of this is to produce in a community a section of women who cannot possibly perform that function for which they were fashioned. Their energies are naturally directed into other spheres, as evidence of which we see the revival of the movement for political recognition. The agitation is no new one, and apparently is dependent for its strength and virility on the position of the sex pendulum," &c.

Now, first, it may be observed that women are no more fashioned to perform a single function than men are; their natures are as complex, their brains as varied as men's—in fact, "God Almighty made 'em to match the men."

Secondly, the excess of females of all ages over males in this country is between one and two millions, while *five* million women earn their own livelihood. Thus a large number even of those who perform "the function for which they were fashioned" are obliged to "direct their energies to other spheres," quite irrespective of any excess of females.

Thirdly, there is no *revival* of the movement for political recognition—it has culminated. Since it first began with any vigour, in 1867, it has gone steadily on, and its greater activity during the last five years has been due to the genius and courage of two women, who had the political insight to realise that, by some curious quality in the psychology of men, the only tactics that are successful in obtaining a reform of the franchise are militant tactics.

Fourthly, the countries in which English-speaking women have already gained their political freedom are *not* those in which there is an excess of women over men, but are the comparatively new countries—New Zealand, Australia, and some of the western States of America.

Dr. Ewart errs in attributing to a purely physical cause a movement which really arises from a mental and moral awakening—and, indeed, his whole article is full of unsupported assertions and loose reasoning; but I should not have ventured to criticise it had he not so clearly allowed his judgment to be warped by his political bias.

HERTHA AYRTON.

41 Norfolk Square, Hyde Park, W., January 9.

I AM sorry that my little paper should have been taken as prompted by political bias. I am sure that its possible influence on the Suffragette question never entered my head. I should be quite willing to answer any question Mrs. Ayrton may care to put to me should she care to write me privately. I am not willing to enter upon a public correspondence. R. J. EWART.

The Health Department, Municipal Buildings, Middlesbrough, January 12.

The Origin of Man.

THE reference in "Dodsley's Annual Register for 1767," mentioned in NATURE of January 12 (p. 336), is to James Burnett, Lord Monboddo, whose speculations as to the simian origin of man excited so much ridicule amongst

his contemporaries. Boswell reports a saying of Johnson in 1773:—"Other people have strange notions, but they conceal them. If they have tails they hide them, but Monboddo is as jealous of his tail as a squirrel."

Burnett's work "On the Origin and Progress of Language," in which these speculations are put forward, only began to appear in 1773, but his views were evidently familiar at an earlier date. He became a Lord of Session in 1764. CECIL H. DESCH.

University of Glasgow, January 16.

[MR. F. GILLMAN, Brook House, Matlock, has sent a letter to the same effect.—ED. NATURE.]

POPULAR ORNITHOLOGY.[1]

IN producing yet another book on the birds of Great Britain [1] the editor points out that one result of the growing interest taken during recent years in the study of ornithology is a considerable addition to our knowledge of the habits of British birds; that as no comprehensive British work on the subject has appeared since those of Yarrell (revised by Newton and Saunders) and Seebohm, this knowledge is only available by searching through a large and scattered literature; that the new edition of the Naumanns' work leaves unrecorded many of the observations on the habits of our birds that have been made in our own and other countries, and that there is therefore place for a work that will bring together from every source, foreign and native, all the available information of any importance concerning the habits of British birds. To do this, and to do it in a form interesting alike to the student of animal life and the general reader, is the chief object of the present undertaking. This is to say the least an ambitious project. In carrying it out the editor will have the assistance of the following writers, J. L. Bonhote, William Farren, the Rev. F. C. R. Jourdain, W. P. Pycraft, Edmund Selous, A. Landsborough Thomson, and Miss Emma L. Turner, who have been left to arrange and treat the matter within each section of a chapter written by them "in the way best suited to his style and temperament, thus avoiding cut-and-dried uniformity with its resulting aridity."

The plan of the book differs in some important particulars from that generally adopted. Each chapter deals, not with a species, but a family, thus not only emphasising the relationship of the species, but facilitating comparative treatment and avoiding unnecessary repetition of statements that apply equally to the whole family or genus. In many cases it has been found advisable to divide the chapter into sections. In the present volumes all the finch genera are taken together "owing to the marked similarity in the general habits of the species," while the crow family has been divided into groups. But when we find the magpie and the jay grouped together for the same reason as the finches and the raven separated from the crows, and all three from the rook and the jackdaw (which are taken together), it is quite evident that "rigid uniformity in arrangement has not been attempted."

The information most often needed for reference is placed at the head of the chapter, under the title of "Preliminary Classified Notes," and refers to each species separately. These comprise (1) description of plumage; (2) distribution; (3) migration; (4) nest and eggs and information as to incubation, number of broods, &c.; (5) food; and (6) period of the year during which the species sings. So far as we can judge from the present instalment, these have been carefully prepared, and contain accurate and concise information, a detailed account, however, of the geographical distribution, as expressly stated in the preface, lying outside the scope of this work, which professes to deal comprehensively only with their habits. Those portions of the chapters treating of the habits generally, and forming the greater part of the volume, are somewhat gossipy and discursive in character, and even bordering in parts on the whimsical, while their popular character may be indicated by a reference to the devotion of two-thirds of a page to such matter as an account of Charles Dickins's ravens.

Mr. Selous makes the startling statement that young goldfinches are not fed apparently more than once in an hour. But in a footnote we are told that the observations (on which the statement is founded) were, it is true, made in the United States, and the Latin name of the goldfinch was not given in the original paper. "Still, it seems probable that what applies to the North American species of goldfinch would apply to our own." Wild speculations on probability of this kind seem to be a waste of space. The "American goldfinch," as a matter of fact, is quite a different bird from our goldfinch, and is closely allied to the siskin. It is a pity that the author of this section did not learn its Latin name. We do not think this portion of the work will supersede our old friend "Newton's Yarrell."

Photo by N. F. Ticehurst.

FIG. 1.—Blueheaded Wagtail's Nest and Young in Grass. From "The British Bird-Book."

The second volume treats of the buntings, larks, wagtails, pipits, the creeper and wren, in the order named, the treatment often inclining to the fanciful. In other places the grouping of the species, often diverse except in name, seems to have raised a slight difficulty, and some species—the shorlark, for instance—might well have received a fuller notice. Of

[1] "The British Bird-Book. An Account of all the Birds, Nests and Eggs found in the British Isles." Edited by F. B. Kirkman. Vol. i., pp. xviii+156; vol. ii., pp. 140. (London and Edinburgh: T. C. and E. C. Jack, 1910.) Price 10s. 6d. net.